REVIEW OF ECOFRAM TERRESTRIAL REPORT

Reviewer: P J Edwards. Zeneca Agrochemicals. Completed 17/6/99

There are some excellent advances for risk assessment in this report and the entire group should be complemented by achieving so much is such a short period of time.

Answer to specific questions directed at Reviewers

1. Is the draft report scientifically sound?

There were a number of high quality and original contributions made by the Terrestrial ECOFRAM group. These principally covered

- Levels of refinement
- The dose from ingestion of contaminated food and PARET
- Granular Exposure Model
- Application of Avoidance to risk assessment
- Interspecific methods and variability
- Probabilistic methodology and its application to risk assessment

The report is essentially scientifically sound. Specific comments/questions have been identified by page and line number.

2. Did the ECOFRAM Workshop address the 'Charge to the Terrestrial and Aquatic Workshops'?

The Terrestrial Group have made some very significant advances in deterministic and probabilistic risk assessment for oral toxicity to birds. The group stated that this represented a model for probabilistic risk assessment to a broader range of terrestrial wildlife. Difficulties in combining pesticide exposure from other routes (dermal and inhalation) were identified for further research. No attempt was made to address the indirect effects of pesticides, even though it was considered to possibly impose greater impact on farmland wildlife than the directs effects. In this case they failed to address their charge. It is a concern that large amounts of resource are directed at testing pesticides to reduce uncertainty in risk assessment when the major effects on wildlife and biodiversity come from agricultural intensification and landscape management.

3. What are the limitations for predicting risk using the approach described?

Without a doubt the limit to probabilistic risk assessment is the amount of data which needs to be generated. This is why the Level of Refinement is so important in connection with having a broad view of what are the most important impacts on farmland wildlife populations and biodiversity.

Through the concept of levels of refinement we should be able to fit a level knowledge (certainty) to the risk assessment to identify what needs to be done. I would like to know why the group did not define Limit tests for a single species by the new draft test methods in a consistent way at screening Level 1.

The list of key concepts agreed upon by ECOFRAM are excellent. In view of the need for probabilistic risk assessment when greater certainty is required there is a clear need for all interested parties to collaborate in providing suitable data to provide generic distributions.

The Threshold of Sensitivity is a key measurement which the group were unable to define. In order for this to be used in the correct and consistent way there is a need to make comparisons with other perturbations in the farming landscape. This will bring us back to requiring a better understanding of the effects of agricultural intensification and landscape management and make us focus on improvement here.

4. What areas of the report need to be strengthened?

I feel the group has indulged in demonstrating they are aware of all sources of uncertainty in both exposure and effects. As a result the 5 day dietary test and 20 week reproduction test has taken more than its fair share of kicks. This has, albeit unintended, created a very negative image of not only past risk assessment but also the potential of probabilistic risk assessment in future, which is not justified. The past 20 years of risk assessment has shown a considerable improvement in management of pesticide safety. The continuous improvement process should not be used as a yardstick of a poor past and present, which is clearly not the case.

The report would be more effective if more of the secondary support data were in the appendices and a summary written.

5. At what point in the risk assessment process is the certainty level high enough to support the consideration of risk mitigation?

I don't see why risk mitigation cannot be applied at any point after screening level 1 providing the impact of the mitigation measures can be measured. The latest point at which it is applied would depend on the Threshold of Acceptability and leaves the choice of mitigation or further risk refinement.

Specific Comments/Questions

1.1 INTRODUCTION

1.7.4 onwards
The Introduction is too extensive. Much of the information is repeated in subsequent chapters of the report, where it should be as it forms part of the groups recommendations. This is particularly so from 1.7.4 onwards. For example the concept of 'Levels of Refinement' while extremely important should not be in the Introduction.

1.7.2 L6-10.
The other titles synonymous with probabilistic risk assessment also address deterministic risk assessment.

2.1 PROBLEM FORMULATION

2.3 Types of Ecological Effects
A ranking of potential risks or sensitivity analysis for farmland wildlife would see Indirect effects endpoints (including changes in agricultural management practices influenced by pesticide use) at the top of the list. While the need for a fresh look at risk assessment with probabilistic methods is valuable it is disappointing that greater consideration was not given to indirect effects because pesticide registration process is constituted to do so.

2.6.4 Identification of Species at Risk
I agree with the groups view that we need to look at 'key species' where factors like PT vary considerably. Did the group consider seasonal changes in diet by 'key species' such that they often function like 'generic' birds consuming close to 100% a particular diet over the medium term at different times of year. This trend may be more a feature of resident/sedentary farmland birds than migrant species.

3.0 EXPOSURE

Fig 3.1.1.
While vegetation is not a significant source of food and therefore source of exposure in birds, this is certainly not the case with some mammals frequenting farmland.

3.3.1 Detailed equations of dose through food.
The model for the dose resulting from the ingestion of contaminated food appears robust but it could lead to a multiplication of uncertainty due to lack of knowledge. It would appear that considerable research effort are needed to understand the individual distributions for key species in key crops.

PT
3.15.L9-10.
Did the group consider the simple equation n mean/Nmin (Fletcher and Greig-Smith 1988) which utilises repeated counts to the same site over a short period (few weeks) as a means of estimating the proportion of time a species spends in a field of a particular crop? Suitable field data may exist from field studies or monitoring surveys. PT estimated by this method assumes presence = feeding and needs checking against more intensive radio-tracking methodology in different crop habitats. A comparison is available for orchards.

TFIR
3-26.L17-22
Nagy made his FMR estimates with free living birds? A 3 fold factor in Level 1 means small birds and mammals will be almost consuming more than their own bodyweight/day in dry weight food . This is unrealistic at any time of year. Seasonal differences may be examined by measurement of FMR for the same species in different seasons.

PD
3-28.
The majority of birds and many mammals adapted to farmland are omnivores. However, optimal foraging theory and descriptions of seasonal diets suggest that birds feed on specific diets chosen because of their high calorific content (or other i.e. protein for nestlings) and availability. The net effect probably being that for several weeks at a time birds may be behaving as either herbivores, graminivores or invertivores. Did the group consider estimating exposure for small, medium and large animals within feeding guilds (herbivores, graminivores or invertivores) at Level 2? This approach is simpler and probably more accurate for short and medium term exposure. I accept that this may run counter to estimating PT where birds in the same guild may behave quite differently.

3-29 L1-6
Did the group consider a possible bias in diet analysis of stomach samples, underestimating the proportions of readily digested food groups in diet.

FDR
Table 3.3.2.
I have used 20, 80, 20 and 30% for vegetation, seeds, fruits and inverts respectively based largely on Zeneca unpublished data which appear consistent with ECOFRAM data.

AVOIDANCE
3-32 & Appendix C2.
The mechanistic approach to understand how avoidance is likely to occur in the field for different species from short-term exposure is excellent. A similar level of understanding for avoidance following long-term exposure would be extremely valuable.

W
3-37.
Bodyweight is influenced through fat deposition by cold weather and migration in healthy birds. For the 'screening level' did the group feel the summer/breeding season weight was appropriate as this when most pesticides are applied?

3.4 Dose resulting from the ingestion of contaminated water

3-39. P - Dew may be an important source of water during periods of drought.

3.5 Dose resulting from the ingestion of granules

GEM is an enormous improvement on the LD50's/sqft hazard assessment and the model appears to take account of all obvious inputs. Granule preference and avoidance must be a powerful factor to influence GIR and should lead the development on less preferred granular carriers. The proportion of grit taken from treated and untreated areas on the other-hand is probably an enormous source of uncertainty. Birds generally forage in optimal ways for resource. It maybe that a much lower proportion of grit comes from treated areas on soil types with little medium and coarse sand than is estimated by PT, which is probably a much better estimate of foraging for food. Can this be examined experimentally in a generic way?

C3-12 States that granules disintegrate and is accounted for by SurTime. Does this imply granules become smaller or quickly completely disintegrate as might be expected for clay. Thus is there a need to consider very small grit/soil particle ingestion, below the range characterised by Best, in bird gizzards or just consider the chemical contamination as part of soil ingestion (3.6).

3.6 Dose resulting from the consumption of contaminated soil

3-54. The inclusion of this compartment should be dependent on sensitivity analysis to see if it really contributes to the overall ingested dose.

3.7 Overall Ingested Dose
3-56. Did the group consider the consumption of one source of dose may affect another. For example the consumption of fresh vegetation may provided sufficient water requirement that no additional water is required. Would the ingestion of soil lower the requirement for grit.

3-60. If exposure through inhalation are required for risk assessment how will they be measured?

3-61.
Did the group consider the EPPO 'Risk in air' decision scheme?

3.7 - 3.9 Overall Ingested Dose

There are many sources of uncertainty listed in these sections due to our lack of knowledge. This presents a negative image. While the oral ingestion of residues on food is widely believed to be the major source of exposure, sensitivity analysis is required to confirm our assumptions and evaluate uncertainty when levels of concern are exceeded. Did the group consider what the level of concern might be in a scale linked to 'levels of refinement'.

3.10 Estimating pesticide concentrations in environmental media

3-66. L21
Data may be available from many sources, however, the quality of such data cannot be assured.

3-66. L23
It is important to know when field data or lab data are being used. There are large consequences to the modelling depending on which type is used.

3-68. L6
This seems to assume that the concentration is the same both on and in the plant.

3-68. L12-13
Assumes that only these two processes are going on and are the same both on and in the leaf.

3-69. L3
This sentence seems a little silly given that the transpiration extracted from each soil layer a given day is part of the sum that defines the total transpiration on a given day.

3-70. L1
This equation seems to assume that there is an even distribution of pesticide over the whole mass of the plant above ground, rather than, say, just the LAI.

3-70. L16
Missing brackets and comma at end of sentence, '$B_{ag}(t=t_i$' should read '$B_{ag}(t=t_i)$,'

3-71. L7
Equation number is wrong, change from '3.4-9' to '3.10-9'

3-71. L22
Other than the models mentioned here, there are many others that do a similar and in some cases better job, *e.g.* PELMO, PESTLA, MACRO.

3-72. L3
Fugacity models are probably not the best approach for dealing with predicting time series data.

3-72. L23
PRZM3 is not the best model to use for this job and as pointed out in the text, there are areas where it needs improving, *e.g.* equations that govern water movement.
3-72. L25

Did the group consider a version of PRZM/EXAMS called MUSCRAT that has been built to run stochastically.

3-73. L2
Is PRZM is the best tool for this?

3-73. L15
EXAMS uses differential equations, it does not generate them.

3-73. L19
See above re. MUSCRAT.

3-72, 22-28
This is an important point. See above- 3-72 L23.

3-76. L8
This is an important point for all the models.

3-76. L17
This seems to assume a fixed relationship and is unchanged over time, *i.e.* independent of transpiration or active processes.

3-77. L9
I don't understand why this sentence says the models use estimated volatilisation fluxes when the previous sentence says the models assume fluxes are zero.

3-78. L15
This is an important point.

3-78. L17
These processes are modelled in PESTLA.

3-90. L16
Again the MUSCRAT model does this.

3-91 & 92 & C-4, 17-18
Probabilistic spray drift calculations for distributions of wind speed and direction can already be calculated in version 1.07 of the AgDRIFT model

3-91.
We are concerned that it might take a long time to develop suitable integrated tools for this probabilistic risk assessment process. Therefore options available for the interim approaches should be considered for short term application after successful validation against real data, before more sophisticated models are developed. An alternative approach would be to attempt developing regression based models based on existing data.

3-92. L25-26..3-93 L1-3
Loss of radioactivity through volatility may be a problem for measuring mass balances in open systems.

3-95. L2

This is a good example of how a model lacking certain processes can have a knock on effect to the processes that it is modelling.

3-96. L24
Auto-correlation between input sets also has to be checked for.

3.10.6. Environmental Databases.
There is no reference to databases for grain/seeds and fruit.

3-98 L5-6. Are data inadequate or is there simply high variability, thus uncertainty?

3-98 L10-11. Existing data also needs to be divided into surface area to weight classes.

3-98 L21-24. Support this very strongly.

3-114 Table 3.12.1. Very useful table which acts as a good summary, but too much information.
Level 1. TFIR x 3 is probably much too conservative.
Level 1. C. Is it better to assume no dissipation in Level 1?
Level 3. TFIR taking account of mixed diets is OK but some birds and mammals feed optimally dictated by availability and need, thus for short periods of a few weeks birds diets will be less mixed than the literature indicates and may be entirely of a single dietary group.

EFFECTS CHAPTER 4

4-9 L3-4
Avian reproduction toxic endpoint should include parental as well as reproductive endpoints. Time scale for the acute LD50 maybe better described as <1 day.

4-9 L22-25
The body burden approach sounds reasonable for systemic toxicity.

4-10. L1
The learning experience form mammalian studies needs expanding.

4.2 Suitability of Current Toxicity Tests

4.2.1 Acute Oral Toxicity Test

4-17. L11-15. Need a single primary endpoint to define doses. The draft OECD guideline considers mortality to be the primary endpoint. The group should consider what problems are associated with setting doses for multiple endpoints in a single study.

4-17. L24-29. Terminology and research. The UDP (Up & Down Procedure) may be used to provide a Dose Response (LD50) or an ALD. Research is in progress to address the utility of the UDP (Chapman et al in prep) to define the dose response curve (with confidence limits) as well as the ALD before a draft guideline on acute toxicity is submitted to OECD. One of the things being considered is placing more concentrations at the thresholds of interest (LD5,10 or 50). We are running simulations to evaluate this. We expect the method to have application beyond birds.

4.2.2 Acute Dietary Test

Agreed, the 5 day dietary test can be improved significantly as the group has outlined and the draft OECD dietary guideline does take account of these problems. However, apart from underestimating the true LC50 when avoidance has occurred it has served risk assessment well. Collection of group mean food consumption over the 5 day exposure period enables an estimate of the dose, albeit sometimes subject to low precision due to inaccurate food consumption and partial group deaths.

There is no mention of 'chronicity' the ratio of the LD50/LC50 (when expressed in mg/kg/day) to indicate the potential to bioaccumulate or eliminate the dose or time quotients. Has the group considered how this might be used in probabilistic risk assessment.

The 5 day dietary test has been 'sacrificed' as it appears expedient to do so. There are many instances where the LC50 has not been underestimated through avoidance of the dose. When avoidance has occurred it is recognised as being indicative of some protection from lethal exposure in the field. So overall it has been a valuable test, but one which can and should now be improved upon.

4.2.3 Reproduction Test

4-20. L28>
The current and new dietary reproduction studies use constant dietary concentrations and has a top rate close to the LOEL . Rather than having a dynamic dose has the group considered measuring remission following cessation of the dose at the LOEL concentration as an alternative.

4-21. L18-23
Agreed, a dose response provides significant benefits compared to a NOEL/LOEL. Did the group consider the difficulty in getting a threshold for all endpoints of primary interest in a single test (i.e. egg laid, fertility, embryo and chick survival). This might only be achieved if doses are suitably spaced, which may compromise the precision dose response curve and confident limits.

The 20 weeks repro study has been sacrificed principally for low power and lack of a dose response. In recent years well conducted studies have delivered a NOEL and LOEL usually at dietary concentrations about a factor of 2-5 apart based upon deterministic risk assessment requirements with the power to detect effects <25%. It is doubtful that a dose response test will achieve much better for all its endpoints (see para above).

4.3.2 Sub-lethal effects

Tucker and Leitske' (197?) published a paper comparing lethal and sub-lethal thresholds with the same chemical/test system and concluded that sub-lethal thresholds would principally fall within a factor of 6x of lethal thresholds. I think this has been shown to be optimistic. However, did the group consider the value of a modern database to look at the generic distribution of different key sub-lethal effects about the lethal threshold to reduce uncertainty without the need to measure sub-lethal endpoints routinely.

4.4.2 Sources of intra-species variability and their relative magnitudes

Table 4.4-1. The data points appear a bit light for 'amongst tests and species categories'. The recorded variability within test and laboratory appears lower than I would have expected.

4.4.2.2 Factors influencing intra-species variability

4-33. L22-23.
Why are younger birds and mammals more sensitive to pesticides? How much of the sensitivity is due to size, which is inversely proportional to exposure (mg/kg/day food) and sensitivity (smaller animals being less able to withstand anorexia than larger animals).

4-33. L26.
Why worry about non-tested species if a distribution is required to estimate the 5%ile.

4-34. L1-2.
Increased food consumption affects exposure and is not related to sensitivity.

4-34. L10-12.
This trend could be due to ability of larger birds to withstand anorexia resulting from the dose.

4-37. L11.
Adverse effects underestimated when the DR is shallow and overestimated when steep?

4.5 Interspecific Methods and Variability

4.5.2 Analysis of phylogenic relationships amongst species sensitivity data

Very well presented.

4-51. L2-6.
To what extent do inter-laboratory differences contribute to the uncertainty ascribed to species. If size influences sensitivity through ability to overcome pesticide induced anorexia there might be higher uncertainty between the same species tested in different labs due to sensitivity to slightly different conditions.

4-69. Table 4.5-5.
Please check table for correct entries.

4-72. L13-14.
Regurgitation may be underestimated as a potential a problem when additional species are tested. As more species are tested we need to build up a database of species less prone to regurgitation.

4-73. Did the group consider whether there is evidence to suggest that sensitivity between species in the short term test (LD50) is predictive of sensitivity in medium and long term tests. Can this be done for the limited number of guideline species?

4-74. L25-26.
Did the group consider the value of a measure of chronicity from the ratio of the LD50 mg/kg / LC50 mg/kg/day as a measure of the temporal nature of toxic dose.

4-75. L4-5.
Parental effects (i.e. egg production) may have the same toxic mechanism as in acute toxicity. Predictions maybe difficult to make but should not be ruled out.
4-75. L12.
Why test 2 species at level 1 for avian reproduction (long-term). Why did the group not consider a similar procedure to LD50 (short term) and dietary LD50 (medium term)? Surely

there is only a need to test additional species when greater certainty is required for risk assessment.

4-76. L5
Can achieving the desired level of certainty lead to less as well as more testing. I think you should always start with a single species and apply desired level of certainty criteria. In this respect has the group considered the acceptability of Limit tests when there is scope to accept high uncertainty when the risk is very low?

4-76. Table 4.6-1.
There is a place in the table for acute endpoints for long-term exposure. Does this make sense. The new OECD 6 week quail study is planned to detect early (first 1-3 weeks exposure) effects from late (4-6 weeks exposure). Would this satisfy an assessment of reproductive endpoints for medium and long term exposure? How is long-term exposure defined? Is it related to the DT50 and multiple applications expressed as the distribution through time?
Furthermore, egg production is a key parental (sub-lethal) endpoint in the 6 week quail study. With effects measured early and late will this fit the requirement for medium and long term other sub-lethal endpoints?

4-78/79/80 Tables 4.6 - General comment

These tables are a valuable way to summarise the conclusions. Can they be made less detailed to get the key points over better?

Did the group consider consistency in defining levels of refinement in tests for short, medium and long term periods of exposure. I do not think consistency is essential but in this case the principles seem identical for short, medium and long term effects testing, although the level of knowledge differs greatly for establishing EF's for interspecies uncertainty. No reference has been made to limit tests. Was consideration given to these as a means to reduce the numbers of animals tested, on the provision that levels of concern are not exceeded. With the addition of Limit tests, the levels might look like:

1. Limit test
2. Dose Response test
3. Test more species depending on EF's (DR or ALD)
4. Probabilistic effects distribution

It is pleasing that the group were able to positively build on the initiatives from the Pensacola workshop/OECD draft guidelines. However, I felt the group could have been more committed to these new studies at Level 1 where the endpoints fit closely with the groups requirements to deliver better risk assessment and release the 'trusted' old studies. I think the Research needs are very important and should be removed from the table and presented in greater detail in the text.

Specific comments on tables 4.6

Table 4.6-2. Short
Toxicity test - why ALD or DR for Levels II & III?

Table 4.6-3. Medium
Effects analysis for focal species the same for Levels I,II and III?

Table 4.6-4. - Long
I see no reason to start with 2 species in the first instance. One species and a limit test should be the first step or it should be optional to start at a more advanced level.

A refined and dynamic exposure regime is mentioned in both tables 4.6.3 and 4.6.4. Does this mean the duration of exposure will be estimated from the DT50 or the dose in any treatment decline with time. The former is good but the latter worries me. Did the group consider measuring the speed of remission after the onset of effects. I think this, together with testing for an appropriate exposure period may be a better way to address more realism than a dynamic dose.

In respect to uncertainty not accounted for, I think an avian repro test could deliver a DR and effects from 3 weeks exposure can be measured in the OECD 6 week study. Effects after 1 week may be possible.

4-81. L22.
'focal' or 'more surrogate' species?

4.81. L25-28.
The UDP may provide a confidence interval more efficiently in terms of no's of animal tested than concurrent dosing (standard test). It may also provide an ALD with poor confidence limits very efficiently which may be suitable for comparison with the LI DR. How good do confidence limits have to be?

4-83. L1-11.
Lack of measurement of sub-lethal endpoint seems to be a source of uncertainty. If there was a generic relationship between the distribution of mortality and ecologically significant parental effects would this be useful. This might be obtained from old LD50, LC50 and Repro studies after adjusting for food consumption to estimate the dose (mg/kg/day) although it would be low precision.

4-83. L13-23.
I think the notion at LIV of using the field to define exposure and the laboratory to define effects is good. Measuring effects in the field in association with exposure would be more a validation process.

4-84. L1-7.
Criteria for triggering a LII medium term study include mechanism of toxicity and bio-accumulation. Did the group consider the use of chronicity here, the LD50 mg/kg divided by the LC50 as an LD50 CR mg/kg/day.

4-84. L9-13
Knocking the old LC50 study again. Too sweeping and repetition. It has proven itself to have been satisfactory for deterministic risk assessment in the past.

4-84. L17-19
Is a dynamic exposure regime the best option? Comment similar for long term repro study above. The exposure period could be set by the exposure distribution through time. If sub-lethal biomarkers are used they could be used to measure remission. Together these may provide a better level of refinement.

4-84. L21-22.

The dose response may be different for lethal and no-lethal endpoints so this may sometimes be difficult in a single study. Therefore a primary endpoint need to defined for setting dose levels, which up to know has been mortality.

4-85 L1-8.
EF's from short term assessments are probably OK. Did the group consider checking how these match with mallard and quail from old LC50 studies assuming that group mean food consumption can be used to provide a measure of dose in mg/kg/day. Furthermore Hill et al (1975) tested 4 species (same spp plus pheasant) and recorded food consumption, but did not present this in the report. Is the raw data available?

4.85 L11-12.
A rather sweeping and negative sentence about the current avian reproduction study considering we have used it for many years in the belief that it has provided protection through risk assessment, especially for those studies which established a LOEL as well as a NOEL. There is no evidence to the contrary. It can be improved considerably by redesign.

4.85. L13.
Why not a single species at LI? (see previous comments).

4.85. L23-24.
Exposure regime? (see previous comment)

4-86. L8-11
These all need further discussion.

5.0 RISK ASSESSMENT METHODOLOGY
Very well presented.

5-39.
Why is the history of application in aquatic toxicology a disadvantage?

6.0 LEVELS OF REFINEMENT FOR THE ASSESSMENT PROCESS

Levels of refinement is a very good concept. I think there is a rather weak natural separation between Levels II and III. The difference is largely between the use of EF's or Distributions (also table 6-5).

Table 6.2.1

Level I. Effects. Why is their no requirement for Limit testing when high uncertainty can be tolerated in risk assessment?

Level I. Effects. Why is it necessary to test 2 species for long term exposure (repro study) when only single species are tested for short and medium term risk?

Level I. Exposure. I am pleased to see a Time Weighted Average at Level I. However, TWA is not included in Table 3.12-1. Needs consistency.
Level II. Exposure. Why have a 'distribution of TWA' when a TWA is the mean over time of a distribution. I think we will loose information by doing this.

Level II &III. Spatial. Treated area, non-target area (does the group mean unsprayed area) and drift zones naturally all fall together (PARET) so is there really a natural separation here.

6-3. L17-18.
Effects are not conservative at Level 1 unless EF applied.

6-7.
Flexibility is good but open to misinterpretation by inexperienced risk assessors.

6-9. L12-13.
'A risk prediction….with adequate certainty'. This is a good case for using limit tests and starting out with 1 species in short, medium and long term risk assessment.

6-13. L10-16.
6-19.
Threshold of Acceptability. I agree this is a sensitive issue. The threshold might be set through an understanding of population dynamics or natural perturbations from which recovery is quick. I can see a benefit of this being agreed by the risk manager because scale of use may be an important factor but as a registrant, I would be worried about inconsistency in decision making.

7.0 RECOMMENDATIONS

ECOFRAM charge

The groups focus on birds, oral exposure and direct effects is understandable. While other routes of exposure…are probably less important; indirect effects in their broadest sense, probably have a much greater impact that direct effects in modern agriculture. This needs to be 'kept in mind' when prioritising resource to fulfil the charge.

7-3 & 7-4.
The key concepts identified by the group fit closely with some International initiatives (i.e. OECD, and EPPO), albeit mostly for deterministic risk assessment. This provides excellent opportunities for harmonisation, which is highly attractive from a registrants perspective. The flexibility in the Levels of refinement may also be attractive for international harmonisation. Did the group reach agreement about the 'threshold of acceptability'?

7-3. L14-17.
Did the group consider approaching the registrants for access to subsets of data or do they feel the databases mentioned are adequate?

7-4 L13-14.
The use of higher levels of refinement to define the uncertainties in screening level assessments is an excellent idea.

7-6. L11-12.
Avian diet comprise seeds/grain, invertebrates (above and in soil) and less so vegetation.

7-7. L10-11.
Territory mapping and point count data (BBS) will provide data to estimate relative abundance and possibly an estimate of PT from the equation n mean/N min (Fletcher and Greig-Smith 1988). The latter would have to be validated by comparing with limited radio-tracking data.

7-8. L1-7
EU guidelines request multiple laboratory and field studies to establish the soil degradation and dissipation kinetics. Did the group consider including non-US studies to broaden the databases.

7-9 L10
Registrants are routinely providing time series crop residue data for registration in the EU. Fate is compound dependent and generic data are probably limited. I think the group should consider this recommendation further.
7-9. L2-3.
The recommendation by the group that EPA and Industry works together to design studies would gain greater support if developed in a Internationally harmonised way.

7-11. L7.
The Up & Down Procedure (UDP) can be use to define an Approximate Lethal Dose (ALD) or even a Dose Response with confidence limits. This is being investigated by a group of statisticians (Chapman, Farrar, Collins, Springer and Slob) as an initiative following on from the Pensacola/OECD avian workgroup activities.

7-11. L24-28.
Excellent recommendation to work with OECD.

7-12. L4-14.
Consistent with OECD Pensacola Avian Workshop

7-12. L21-22.
Better estimate of LD10 being undertaken by Stats group.

7-12. L29.
Is a dynamic exposure regime necessary if duration of exposure takes account of the DT50 and remission measured if a relevant sub-lethal effect/biomarker is measured?

7-13. L27-28.
Does the group consider that the effects of metabolism and depuration are implicit in the effects tests? Can an estimate of chronicity from short and medium term studies expressed as mg/kg/day provide the necessary information?

7-14. L23-28.
Some validation will develop from commercial use, through incident reports and maintenance of population indices (i.e. BBS). This may be too slow a timescale and the latter is effected by indirect effects etc.

7-16.
I hope that EPA will find a way to developing CRADA with the aim to pursue harmonisation through OECD.